FROM SPECULATION TO EMPIRICISM IN THE STUDY OF MENTAL DISORDER: RESEARCH AT THE NEW YORK STATE PSYCHIATRIC INSTITUTE IN THE FIRST HALF OF THE TWENTIETH CENTURY

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INTRODUCTION

From its inception in 1896, the New York State Psychiatric Institute (P.I.) has been devoted to the dual function, in varying degrees, of research and clinical work in the field of mental hygiene. Although the continuous history of psychological research at the Institute starts with the arrival of Carney Landis in 1930 when the Institute moved to its present quarters at the Columbia-Presbyterian Medical Center, there have been strong undercurrents of theoretical interest providing a basis for experimental investigation of the psychological factors involved in mental disorder during the entire history of the Institute, and these undercurrents occasionally surfaced in the form of specific programs. One such period came with the founding of the Institute when Ira Van Gieson, the director, and Boris Sidis, the psychologist on the staff, conducted their psychopathological researches. Another occurred ten years later when the Kent-Rosenoff team at the Kings Park State Hospital and F. Lyman Wells at P.I. made significant contributions to psychological testing through their work on the word-association technique. A third instance took place years later when David Wechsler advanced the testing of memory in his study of retention in Korsakoff psychosis. Soon after this, Nicholas Kopeloff refuted Henry Cotton's focal infection theory of mental disorder with a carefully controlled experiment that profoundly influenced later evaluation of therapy and the study of mental disorder in general.

One of the most important theoretical undercurrents during the early years of the Institute was an interest in observable phenomena which led to objective and thorough recording of patient behavior on a sound phenomenological basis, originally propounded by Adolf Meyer and nurtured by August Hoch, George Kirby, and succeeding directors. This interest—although clinically oriented in that the treatment of individual patients was the primary objective—provided an indispensable theoretical prerequisite for modern quantitative evaluation procedures.

VAN GIESON'S PATHOLOGICAL INSTITUTE: THE "NEURON ENERGY" MODEL OF PSYCHOPATHOLOGY

The New York Pathological Institute (the original name of the N.Y.S. Psychiatric Institute) was founded in 1895 by the state legislature under a mandate from Carlos MacDonald, then the president of the State Commission in Lunacy, calling for a "laboratory . . . operating as a coordinating and processing center for the pathological work of the state hospitals (Ref. 1, p. 22). Dr. Ira Van Gieson came to the institute as its first director with a background in neuropathology, having been associated with the pathological laboratory at the Columbia College of Physicians and Surgeons. He designed a multidisciplinary research program for the institute that went far beyond Dr. MacDonald's original mandate and exceeded the bounds of his own area of specialization, a program coordinating work in fields as diverse as comparative neurology, psychopathology, psychology, bacteriology, and (physical) anthropology in the study of mental disorders. Van Gieson found encouragement for his ambitious program for the Institute in his association and friendship with Boris Sidis, a student of William James who was interested in hypnosis, and a dissociation theory of mental disorders that stemmed from Janet's work with double personalities. Sidis elaborated this line of theoretical speculation to include the "moment consciousness" contributed by each neuron to a total mental state.

Together, Van Gieson and Sidis refined both the neurological and the psychological aspects of this speculative trend to provide an accounting for clinical observations of normal and abnormal behavior on the basis of a dynamic model of neurophysiology. In 1898 they formally presented their neurophysiological model to the profession in an article entitled "Neuron Energy". Neurons, they claimed, must maintain a certain level of "dynamic energy" in order to conserve their position in a "neuron aggregate," and the position of this aggregate in an "associated cluster." If the energy level in a particular neuron drops from the dynamic level to a level of "static energy," the neuron aggregates begin to dissociate from each other, and the individual neurons then tend to separate from their aggregates. With further loss of energy a lower level of static energy is reached, resulting in degeneration of the neuron. If the energy falls to the lowest organic level, then the neuron is destroyed.

The energy level of neurons is associated with the alternation of "sleeping" and "waking" states. Ultimately, this energy balance is dependent on the supply of food:

If there be one all-important question in the production of insanity, it relates to the balance between food supply of the nerve cells and the work performed or withdrawal of nervous energy [italics his]. This is a practical question, because every one knows that if more energy is drawn off from the nerve cell than can be produced from its food supply, the result is bankruptcy of the nervous system. (Ref. 2, p. 82)

‡A concept reminiscent of the "cell assembly" construct developed some 50 years later by Donald O. Hebb.

*This paper is based on the monograph "Seventy-Five Years of Research in the Behavioral and Social Sciences at the New York State Psychiatric Institute," written in conjunction with the 75th anniversary celebration of the Institute. Some sections of this paper are based on varying degrees on the annual reports of the New York State Psychiatric Institute and Hospital, which will not be specifically cited in the text. These reports have been issued over the years under the following titles: Annual Report of the Director of the Psychiatric Institute and Hospital to the Department of Mental Hygiene (1930-present); Annual Report of the Director of the Psychiatric Institute to the Commissioner, Department of Mental Hygiene (1927-1929); Annual Report of the Director of the Psychiatric Institute of the New York State Hospitals to the State Hospital Commission (1912-1926); Annual Report of the Director of the Psychiatric Institute of the New York State Hospitals to the State Commission in Lunacy (1908-1911); Annual Report of the Pathological Institute of the New York State Hospitals to the State Commission in Lunacy (1896-1907). All other sources are cited in the text and bibliography.
The balance of food supply in the nerve cell and expenditure of energy is adversely affected by two factors. One is overexertion:

Any one may see this in his daily walks of life in the man who overworks and overfatigues his nervous system. We see this bankruptcy of the nervous system everywhere about us in the endeavor to cheat time in the pressure of hurry and haste in the activity of large cities. People expend more energy from their nervous system than they supply through food and rest. (Ref. 2, p. 82)

The other factor affecting the food supply to nerve cells is summed up by Van Gieson under the rubric “poisons”:

the poison of syphilis and the chronic and persistent poisoning of the body by alcohol, both of which seem to operate largely by diminishing quantitatively or qualitatively the food supply of the body cells . . . . cause degeneration of the nerve cells . . . . (Ref. 2, p. 119)

The hypothetical changes of neuron energy level and the resulting hypothetical dissociation, degeneration, and destruction of neurons and their aggregates correlate, in Van Gieson’s theory, with clinically observed malfunction in various mental disorders. For example:

The phenomena of psychopathic anaesthesia, aboulia, amnesia, psychopathic fixed ideas, recurrent dissociated moments, multiple personality—all form an inexhaustible mine for the study of the laws and relations of mental processes and the various types of mental activity, and we may arrive at the important generalization that functional psychosis is a disorganization of psychophysiological constellations and dissociation of moment-consciousness accompanied by a corresponding dissociation, or possibly retraction, of neuron systems. (Ref. 5, p. 10)

Broadly speaking, psychopathies run parallel to the phenomena of retraction and expansion of aggregates of neurons, while neuropathies are concomitant with actual degeneration of the neuron, especially of its cytoplasm. (Ref. 4, p. 11)

The parallelism of mental and physical phenomena provided Van Gieson with his primary reason for establishing research laboratories in a variety of disciplines and insisting that their work be interrelated. He pointed out

the incongruity of setting forth the claims of any of these departments of the institute investigating insanities as distinct, isolated methods of research. They must all be linked together and work hand in hand. A concrete example of this is the apportionment and yet linking together of the work in the departments of psychology and normal anatomy of the nervous system . . . . Working conjointly, the psychologist and the anatomist show, in an ideally scientific way, the states of the parallelism of the physical process in the nerve cell and the corresponding psychic phenomena. [italics his] (Ref. 2, p. 74)

Van Gieson refers to the correlation between physical process and psychic phenomena as “states of parallelism” with deliberate intent. He notes a fundamental distinction—the unbridgeable gap between mental and neurophysiological phenomena—that has been a perennial source of difficulty for psychology:

The most perfect knowledge, even down to the understanding of the very molecules of the nerve cell, would not help the anatomist or the chemist to postulate the laws and phenomena of thought and consciousness, for these are not products of nerve cell activity. The brain does not secrete thought, as the kidneys secrete urine; thought is not a material thing; it can neither be weighed nor measured. A sensation of color, for instance, as experienced by the eye, has no material existence in the physical world. We can only speak of the phenomena of consciousness.
as running parallel or being concomitant with the workings and metabolism of the nerve cell, lest we drop into the pitfall of the materialistic basis of consciousness, which has been utterly abandoned long ago. [italics his] (Ref. 2, p. 73)

It must be borne in mind that Van Gieson and Sidis postulated these neurophysiological processes; they did not observe them. Nevertheless, they did consider their claims about retraction and degeneration of neurons, and about the integral importance of food supply to the neurons, to be empirical. Van Gieson and his colleagues adapted cell-staining techniques of others and elaborated some of their own so that they could study brain tissue from animals that had been subjected to trauma, food deprivation, and various poisons. They hoped to find the effects of fatigue in the arrangement of nerve cells, but all they were able to observe were gross lesions.

This was also true of their study of human brains. The only stage at which Sidis and Van Gieson could make an actual correlation between their neurophysiological model and the clinical behavior of human beings was when actual "degeneration" of nervous tissue had taken place, as in general paresis:

The earliest manifestations of general paresis, for instance, are those corresponding to the liberations or restorations of the uppermost levels of static energy, but finally the process of liberation reaches such a depth that the disease becomes destructive. (Ref. 4, p. 13)

The particular fact that Van Gieson and Sidis were wrong in attributing the clinical manifestations of general paresis to the spontaneous loss of nerve cell energy rather than to the destruction of cells by the spirochete is less symptomatic of a general failure of their scientific method than of the limitation of the knowledge of the day; their dynamic model of neural processes was untestable in view of the technology they had available; thus they had no way of knowing whether their claims about general paresis, or any other clinical phenomena, were on the right track. Their model of neuron energy in effect provided metaphorical descriptions of clinical phenomena, rather than standing in an explanatory relationship to them.

That Boris Sidis and Ira Van Gieson were not alone in their attempt to provide a neuronic basis for behavior is clearly demonstrated by Freud's attempt in this direction about the same time in his famous "Project for a Scientific Psychology." Apparently, the physicalism introduced by physiological psychology from Helmholtz to Wundt and their followers constituted a permeating feature of the zeitgeist, and Freud's exposure to this mode of thinking at the Physiological Institute at Vienna under the aegis of Ernst Brücke led him to develop the "Project." The underlying common denominator of this movement was the belief that only physical-chemical forces are active within the organism and that appeal to extraneous forces was unnecessary to explain behavior. The neurons were the units that carried these physical-chemical forces. Van Gieson and Sidis on the one hand, and Freud on the other seemed to be imbued with this belief, and their attempts at implementing it through theories of neuronic conduction reflect their varying approaches. But whereas Van Gieson and Sidis remained adamantly committed in their pursuit, Freud gave up the idea when he found it impossible to explain psychopathology on this level. He resorted to a postulation of new forces beyond the physical-chemical inherent in matter, forces that could be of equal stature in producing attraction and repulsion tendencies in man parallel to the attraction and repulsion forces in inanimate matter. This eventually resulted in his anatomy of personality, i.e., the forces of the id, ego, and superego in relation to conscious and unconscious behavior. Although Van Gieson and Sidis matters as hypnosis and dissociation after leaving P.I. and returning to Massachusetts.

The department of psychology and psychopathology was well outfitted with instruments—such as spylmographe, cardiographs, pneumographs, chronographs, ergographs, and reaction-timers—for the purpose of measuring the psychophysical manifestations parallel to nerve-cell phenomena. Sidis seems to have been the main impetus behind the introduction of these experimental techniques, and in addition made use of psychological tests. For example, in a study of prison inmates he suggested that memory be tested by using recall of short series of numbers and letters, both written and spoken. This technique was not as popularly used then as now.

The Anthropology section, under the direction of Dr. Alois F. Hrdlicka, was well integrated into Van Gieson's overall plan for the coordination of research activities at his institute. The neuron energy model, in viewing mental abnormality as a function of cell deprivation and destruction, would predict concomitant deprivation of the "germ plasm" and, as a result, the passing on of mental abnormalities from one generation to another. Thus, Van Gieson's and Sidis' theory concurs with many of the notions of "degeneracy" attributed to mental abnormalities during the nineteenth century and earlier; in fact, it may be regarded as the most sophisticated attempt to integrate these notions into a scientific model. Van Gieson expressed his views of heredity and its relation to psychopathology as follows:

We can now have some glimpse of how immutable are the laws of heredity. This material—the germ plasm—transmitted in equal amounts from both parents to the new individual, will surely pass on damages incurred by the ancestors. If a man exposes his germ plasm to the poisonous influences of alcohol, or still worse, syphilis, such damage is not confined to his individual life only but passes on to the next generation... What are the agencies which damage the germ plasm and cause departures from its normal constitution? Precisely the same agencies, to a certain extent, which cause degenerations or induce disease processes in other cells of the body besides the germ cell. These agencies may be summed up under poisons and factors which deprecate the food supply of the body cells. (Ref. 2, pp. 84-85)

Slight damage to the germ plasm, according to Van Gieson, tends to be manifested in the most complex mental functions, rather than on a lower level:

According to the degree of pathological changes in the germ plasm do the defects of development of the progeny pass successively from higher to lower and lower planes of organization in the nervous system so that all grades of degeneracy and mental instability may be witnessed down to the weak-minded imbeciles and idiots. (Ref. 2, p. 119)

This progression "from higher to lower... planes of organization" was associated, for Van Gieson, with the fact that some disorders seem to appear first in adolescence. He explained the association this way:

During childhood such inherent incapacity of the energy of these higher parts of the nervous system does not always appear, unless the hereditary effects due to damage of the germ plasm be gross and severe, for at this period such higher centres are comparatively little used. During adolescence and later life, however, when these higher centres of the nervous system are called upon for the greatest and most extensive expenditure of their nervous energy they may fail. (Ref. 2, p. 120)

This progressive degeneration in the individual is paralleled by progressive damage to the germ plasm of the offspring. If we consider the "germ plasm" as the reproductive factor, the idea of degeneration becomes more intelligible, for in this way we may explain the increase in degenerative diseases in the succeeding generations.
It becomes worse in the next generation for the reason that this unstable brain energy in the first generation is liable to cause the individual to commit excesses. ... Degeneration of the germ plasm once established tends to set up a vicious circle increasing the degeneration in each successive progeny. The third generation becomes still more unstable in the energy of the higher portions of the brain which hold the lower ones in check. It is from this or succeeding generations that are recruited the inmates of the prison, of the lunatic asylum, of the reformatory and of the hospital for the epileptic. (Ref. 2, pp. 120-1)

Among the roles that the Anthropology section was to fill were identifying degenerates in a population and tracing the progress of degeneracy in an individual and across generations. For the first role the section obtained a variety of instruments for measuring body proportions. Van Gieson indicated that because “the population is so heterogeneous” in this country, careful preparation involving the standardization of measurements on normal individuals would have to be done.

Some research in tracing progressive degeneration during adolescence was conducted by Hrdlicka at a boys’ reform school. He tried to correlate various measures of physical growth with performance on sensory tests, with a particular eye to finding corresponding abnormalities of “phases of degeneracy” in both areas. Henry Lyle Wight studied a group of inmates of desert asylum at the Kings County Penitentiary, comparing their social background with general clinical observations of behavior and anthropometrical measurements. He found “hereditary” factors such as lack of religiosity in the parents, social factors such as the parents’ drinking habits, physical factors such as a slight or marked asymmetry in the head contour, and “psychical” factors such as a lack of “moral sense” and “remorse” in his subjects. All of his correlatives seem to have been impressitionally formulated and determined, except, of course, for the anthropometry.

Van Gieson hoped that there would be a correlation between a history of degeneracy in the family of an individual and his prognosis: ... one prominent purpose of anthropology at the Institute is to ascertain the proportion of cases of insanity occurring in normal individuals, (i.e.) in individuals who have no hereditary predisposition toward insanity—and to compare this proportion with the other cases of insanity complicated with or resulting from hereditary predisposition. For in the former class of cases insanity is more or less of an accident, and in the great majority of cases recovery is to be expected; whereas in the latter class with predisposition recovery is much less liable to occur.2 (p. 123)

It should be remembered that the hypothesis of damaged germ plasm ante-dated the rediscovery of Mendel at the turn of the century. Although much of Van Gieson’s and Hrdlicka’s theorizing now sounds quaint and out of date, they were right in step with the prevalent Lamarckian theories of degeneracy caused by injury to the germ plasm of the parents.

Some of the work on genetics emanating from the mental hospitals of New York a decade and more later, including publications from Manhattan State Hospital no doubt influenced by the Institute, have a more modern ring. Thus, Rosanoff (Kings Park State Hospital) on “The inheritance of the neuropathic condition” and on “Dissimilar heredity in mental disease,” 16 Smith (Manhattan State Hospital) on “Psychoses in twins,” 10 and H. H. Laughlin (Kings Park State Hospital) on “The diffusion of defective traits,” 11 constitute the beginnings of modern genetic research at P.I. and in the state hospitals system.

Van Gieson’s plan for a “correlation of sciences” was ahead of its time in two senses. First, there was as yet no widespread support in the state hospital system and in the legislature for an institution devoted only to research supported by public funds; indeed, the development of this support was to take a full generation. Within a few years after his appointment, Van Gieson ran into heavy fire from the state hospital superintendents and from the State Commission in Lunacy, which had appointed him. They conceived of the Institute as a training center for state hospital physicians and as a central laboratory facility for pathological work. But Van Gieson was adamant in his refusal to modify the original plan. He did not want to dilute the energies of the Institute by trying to teach state hospital physicians, who, to his mind, were incompetent as scientists. He also felt that routine pathological work belonged in the state hospitals, leaving the Institute free for research. His program was defended by the neuropsychiatric research committee outside the state hospital service, but in spite of this support, the political pressure emanating from the Commission in Lunacy and the legislature won out, and he was forced to resign. Schulman, 1 in a thorough and insightful review of this period of the Institute’s history, points out that conservative opposition to Van Gieson’s plans, rather than lack of faith in his abilities, was the reason for his removal:

The burden of the Commission’s, and ostensibly the Legislature’s case against the Institute was not that its research program was poor, i.e., professionally illegitimate, but rather that such a research establishment had no place in a state hospital system which required specific services if it were to meet its obligations to the state’s insane and taxpayers. (p. 25 fn)

Secondly, models of psychopathology and experimental techniques had not yet been developed to the point where experimental work in different disciplines could be coordinated on a sound empirical basis. Van Gieson and Sidis could only conjecture, as pointed out above, about the relation of hypothetical neuromorphic processes to clinical phenomena. Thus it has remained of Van Gieson’s plan, in the passing of time, is his vision of coordinated research in a spectrum of physical, biological, and social-psychological disciplines, a vision which was to be gradually realized more than a generation later in the modern period of the Psychiatric Institute’s development.

Some of the weaknesses in Van Gieson’s background and approach were highlighted by the man to succeed him as director, Adolf Meyer. Meyer felt Van Gieson’s basic flaw to be a lack of practical experience in clinical work, resulting in a tendency to hypothetical abstraction rather than to deal directly with the complex symptomatology at hand. He recounts his first encounter with Van Gieson in 1895:

He showed me a little booklet in which he had noted 48 mainly neurological and biological problems to be worked out. And out of the elements he made, in a remarkably comprehensive manner, his picture of the Correlation of Sciences, in which there were unfortunately two fatal gaps: he had no place for man as we know man, i.e., not as a sum of elements but as a real specific unit; and he had no place for psychiatry as we know psychiatry at work. (p. 5)12

In his criticism of Van Gieson’s approach, Meyer revealed the direction in which his own theoretical interests lay:

By sparing ourselves the trouble of discussing plain facts in terms of brain-cells and hypothetical brain centers, unless we prove them or really further our activity by the hypothesis, we save ourselves time for a better functional analysis of the facts and the heater unnecessary and harmful mystification.13 (p. 327)
ADOLF MEYER AND THE RISE OF PSYCHOGENESIS

Reorganization of the Institute under Adolf Meyer marked the beginning of a primarily clinical phase in its history. Meyer stressed clinical work both because he felt it had been neglected ("the aim of the Institute lies chiefly in the raising of the standard of medical work in the state institutions") and because of his belief that progress in studying the cause and prevention of mental disorders would have to be based on a foundation of careful and thorough clinical observation. His interest in clinical work was such that he made the removal of the institute to Ward's Island, adjacent to Manhattan State Hospital, a condition for his acceptance of the directorship.

Meyer's thoroughly common sense attitude toward psychiatry and hospital administration bore fruit in major improvements in patient care and treatment. As such, this clinical aspect of his work as director of P.I. from 1901 to 1910 may be considered his main contribution to psychopathology and has been recognized as such. However, the fundamental importance of his thinking for the course of psychological and social research in psychopathology has generally been underestimated. Theodore Lidz emphasized that because of Meyer's obscure writing style, his conceptions and theoretical orientation passed directly into practice through his teaching and "became so solidly incorporated into the body of American psychiatry that they seemed self-evident (p. 321)." Nevertheless, a careful reading of his papers reveals his theoretical trend of thought—usually framed in terms of practical application and treatment—to be often strikingly original. Indeed, he should be considered one of the originators of the modern pluralistic perspective in which psychopathology is viewed through a wide-angled lens ranging from the ecological to the developmental, learning, genetic, internal environmental, and neurophysiological models. The attention paid to these multiple insights into psychopathology is reflected in his mapping of the "psychobiological personality," an example of which is found in Figure 2, his well-known "life chart" technique for representing the developmental history of an individual, illustrated in Figure 3, and his use of the now standard genealogical diagram for charting the family history of an individual, which he borrowed from Charles Davenport.

Walter Freeman expresses his evaluation of Meyer's position in the theoretical climate of psychopathology at the turn of the century in this way:

Meyer was first an anatomist and pathologist, and with this background he decried efforts of theorizers to explain mental disease on anemia or hyperemia of the brain, or changes, largely reversible, in the appearance of nerve cells in the cerebral cortex and elsewhere; and on hypothetical toxins, or deficiencies that could not be supported by any findings in chemistry, physics, or physiology. He was equally dubious of the formulations of Freud, Jung and others of the psychoanalytic school.

Meyer seems in fact to have taken a "laissez faire" attitude toward psychodynamics, indicating that he was in sympathy with some general precepts of the trend, if not with many of the extreme formulations. This sympathy was particularly marked during the infancy of the psychoanalytic movement in the United States. A young colleague of his on Ward's Island commented:

Although Meyer understood psychoanalytic theory he seemed never quite able to reconcile himself to some of its fundamentals, especially the examination of sexual aberrations and the early sexual trauma in the development of neuroses. So whereas Meyer never completely accepted Freud, he did not reject him. He credited psychoanalysis with having a new and searching light upon psychotic syndromes.

Zubin & Zubin: From Speculation to Empiricism

FIGURE 2. Adolf Meyer's schematic outline for his "psychobiological" concept of personality, reflecting an interest in ecological, internal environmental, and neurophysiological aspects of psychopathology. (p. 270) (By permission of the Johns Hopkins University Press.)
HEREDITY: PATERNAL UNCLE ALCOHOLIC. MATERNAL GRANDFATHER INSANE. ONE BROTHER HAD TWO DEPRESSIONS.

FIGURE 3. Meyer's developmental interests: the life chart of a case of schizophrenia.15 (p. 54) (By permission of the Johns Hopkins University Press.)

Meyer's “psycho-biological” approach has special importance for modern psychological research in that it contains the seeds of a behavioral model:

Without disputing the views that mental disorders are essentially physical disorders, I maintained that many of the most potent factors leading to dementia praecox cannot be expressed in any other terms than terms of conduct and behavior, and that by singling out the fundamental determining elements of mental life, the history of conduct and of adjustment, a sane and useful psychology could be shaped. That disorders like those constituting the prodromal stage of dementia praecox could be best expressed in terms of habit disorders... has been shown more and more by a careful study of these cases. With this principle as a foundation, the way was opened to a broader conception of dynamic psychology, i.e., a broader valuation of mental events and their possible role in the explanation of mental upsets... Mind, like every other function, can demoralize and undermine itself and its organ, and the entire biological economy... 19 (p. 4) [italics his]

He carried through with this awareness of what psychology could contribute to the study of psychopathology, requesting in the annual report for 1901-2 “an additional department of psychology in order to make accessible the methods of modern psychology.” This request did not materialize, however, as a concrete program during his tenure as director of the Institute.

Much of his interest in the social aspects of mental disorders stems from the influence that Jane Adams and Julia Lathrop exerted on him while he was still at Kankakee State Hospital near Chicago, Ill. They came to close contact with him when he slipped on the ice and broke his leg near Hull House, in Chicago, and was invited to stay there while recovering. Meyer apparently had a close relationship with Julia Lathrop; Mrs. Meyer and he were so taken with her that they named their daughter after her. Mrs. Meyer herself pioneered in the field of psychiatric social work and helped her husband in his study of families. She would make home visits, gathering the background information necessary for Meyer's comprehensive case studies. Thus Meyer and his wife introduced the home environment as an etiological factor in the study of mental disorder.

Adolf Meyer's role as an early proponent of a “dynamic psychology,” favoring attention to the content of symptoms, is not as generally recognized as his clinical work. Although, as pointed out, he was not a Freudian and did not accept much of psychodynamic interpretation uncritically—his own “psychobiological” concept viewed psychological reactions as merely the most complex type of biological, i.e., physiological reaction—he did establish two conditions that promoted the development of psychodynamic theory by others. Firstly, he insisted on a break with existing classifications of disorder based on notions of “exhaustion” and “auto-intoxication,” turning rather to a careful analysis of the history and symptom picture of each patient as an individual.

The old-fashioned symptomatology dealt with psychology in a manner which did not inspire the physician to expect from psychopathology anything beyond the definition of some terms like illusion, hallucination, delusion, compulsion and perhaps depression, excitement and dementia. Misconceptions about psychology, together with the ease with which impersonal physical explanations, such as degeneracy and auto-intoxication, could be put forth as the “facts” supposed to underlie what would otherwise involve one in tedious observations and difficulties of explanation, and perhaps in investigation of the private life of families [i.e., Meyer's approach], had led to the widely spread dogma that mental disorders were “physical” disorders.19 (p. 3) [parenthetical inserts ours—DZ & JZ]

†Lidz notes that “‘habit’ for Meyer had the broad connotation of organized patterns of behavior and thinking, much as Pierce used the word.” (32 fn)
This essentially phenomenological method produced detailed and accurate case studies that provided the raw material for psychodynamic interpretation. Secondly, he accepted psychogenesis in principle as a valid explanation for disordered behavior.

Psychogenic disorders are those which depend on conditions or events which can only be described satisfactorily in terms of psycho-biology; actions, emotional reactions, attitudes, and intellectual or "thought" constellations, and their conflicts and abnormal combinations or atavistic or fundamentally or directly abnormal reactions, with their effect on the general mental balance. Every mental activity or reaction leaves its engram and has a certain dynamic value in the after-life of the individual and his general economy. 10 (pp. 227-8)

Meyer credited Kraepelin for providing the initial impetus for this theoretical development through his "fundamental shaking up of tradition, the declaration of independence and the right of seeing things according to their medical importance." 13 (p. 333) But on the other hand, he became increasingly critical of Kraepelin's formulations, because he

"Found that the symptoms manifested by many so-called "functional" patients fitted neither of the great groups of non-organic mental disorders, dementia praecox and manic-depressive insanity, that Kraepelin kept shuffling and reshuffling in his efforts to establish them as definitely separate entities. ... Meyer reiterated that a full collection ... of "all the facts" in the life history of a patient should always be assembled. He and his assistants found that the data corresponded so remotely with Kraepelin's elaborate descriptions that he felt obliged to acknowledge this insufficiency. 17 (pp. 83-94)

During the first decade of the century other physicians destined to play an important role in the development of psychodynamics at P.I. began to form a collaborative group. C. Macfie Campbell, who had studied with Kraepelin, and in addition was interested in symbolic interpretation of clinical data, first came to P.I. in 1904 for a few years to work with Meyer, and then returned as Associate in Clinical Psychiatry in 1908. George H. Kirby came to Manhattan State Hospital at about the same time, and soon became Director of Clinical Psychiatry there. August Hoch during this time was at the Bloomingdale Hospital, where he had developed an early interest in psychogenic interpretation and in psychoanalysis. In the 1907 annual report of the Bloomingdale Hospital he wrote that

mental disorder is nothing more than the becoming dominant of such conflicts in various forms of peculiar abnormal reactions and faulty attempts at adjustment. These manifest themselves in delusions, hallucinations, peculiar acts, and the like. In each case, therefore, a careful psycho-analysis is necessary ...[leading to an] understanding of the actual struggles and difficulties which are hidden under the perplexing array of mental symptoms. 20 (p. 386)

A colleague of Hoch 17 reports that his interests along these lines went back to his work at the McLean Hospital from 1897 to 1907.

Meyer and Hoch formed the nucleus of a group at Ward's Island, which, beginning about 1908, comprised the first state hospital center using psychoanalysis "day in and day out to clarify the psychiatric syndromes of committed patients ... at a time when the two leading clinics of Germany ... had ignored Freud's work. 17 (p. 81)

C.P. Oberndorf, a resident at Manhattan State Hospital during the formative period of this group, stressed the strong influence of close interaction among the members of the group on the development of their thought.

The presence of many younger men, with their enthusiasm and varied interests in psychoanalysis, living together on Ward's Island afforded mutual help in the professional conversation at meal time and after-hour discussions. The interchange of experience fostered a communal striving to learn the essentials of the psychogenesis of schizophrenic, depressive, and above all, the mixed forms of neurotic illness. These zealous endeavors were reflected in case presentations at the State Inter-hospital Conferences (and at meetings of the Ward's Island Psychiatric Society) ... Interest in psychoanalysis at Manhattan State did not represent a strife against authority ... but conformity to it. 17 (p. 89)

The minutes of one such meeting, containing formal presentations and informal commentary, illustrate the cooperation and sense of purpose among the members of the group. At the quarterly conference of state hospital superintendents in January, 1909 21 Campbell gave a paper entitled "Psychological Mechanisms with Special Regard to Wish Fulfillments." In it he concluded that:

We are now ready to pay more attention to what the hallucinations and delusions really are and what they mean ... we wish to have the actual description of the odd behavior, which we know prognostically to be ominous, and to find out from what chain of circumstances this behavior was derived; we are no longer satisfied with "a priori" statement that such reactions are meaningless, are due to the mere disordering activity of the nervous structures, and therefore not susceptible of psychological analysis. ... Thanks to the teaching of Meyer we now approach our cases freed from the incubus of this assumption, willing to accept the facts in their entirety. (p. 22) One of the factors, which make such an analysis difficult, is the fact that, when the mind is not playing its normal directing role in the adjustments of practical life, thought at a lower level tends to be less clear, to identify objects on account of superficial resemblances, to represent in a plastic and symbolic manner what would have more direct and logical form. (p. 12)

Meyer, in the ensuing discussion, cited the papers as an example of "determining the actual difficulties and tangles of the patient's mental working and effecting a setting aright of the otherwise unguided mental drift." (p. 24) Meyer's comments exemplify his habit of always couching his words in strictly practical terms of diagnosis and treatment, although underlying this the theoretical trend of his thought is evident. Kirby, who was also present, observed that:

Cases analyzed in this way are far more interesting and instructive than if dealt with in the usual formal descriptive manner. It is a safe and practical plan of work because we are getting at the real difficulties and conflicts which the patient has had to meet and over which he has apparently gone to pieces. To understand the mechanism by which these painful experiences become moving forces in the individual's life and give form and color to the psychosis is a most welcome addition to our knowledge. We cannot deny that psychogenetic factors play an important role in the etiology of mental disorders and as such they deserve our closest attention. (p. 26)

Hoch was not present, although he frequently attended these meetings. During the discussion of Campbell's paper a member of the staff directed the attention of the participants to the work that Hoch was doing along those lines at the Bloomingdale Hospital.

The effect that these men were having in establishing a climate in which psychodynamics could flourish is evidenced by the remarks of other physicians at staff meetings. For example, M. J. Karps of Manhattan State Hospital summed up his review of a case of "dementia praecox" at a meeting of the Ward's Island Psychiatric Society in 1909: 22
What does the record of this case teach us? First the importance of psychogenic factors in functional psychoses. Second, that psychic and neurotic symptoms are governed by complexes which originate from definite underrunners of patients’ mental lives. Third, the profound teachings and methods of Freud, Jung and Bleuler should receive our serious attention for we are indebted to them for the true progress of psychopathology (p. 227).

These four men—Meyer, Hoch, Kirby, and Campbell—set the stage for the development of psychodynamics at P.I., but their major achievements in this area, except for Kirby, were destined to be carried out elsewhere. Meyer left in 1910 to become founding director of the Phipps Clinic at the Johns Hopkins University Medical School in Baltimore. Campbell left a year later, to eventually become medical director of the Boston Psychopathic Hospital. August Hoch, who had been able to associate with this group from his position at Bloomingdale Hospital for years, replaced Meyer as director of P.I. His own contributions at P.I. were to be cut short by early retirement in 1917 because of ill health.

In January, 1913 Hoch brought John T. MacCurdy to the Institute staff. His acute interest in symbolic interpretation is shown by summary comments in a paper published in 1914 entitled “A Psychological Feature of the Precipitating Causes in the Psychoses and its Relation to Art”.

In attempting recently to analyze the precipitating causes of the functional psychoses, we have been struck by the frequency in which an apparently innocent from a moral standpoint has been the occasion of a mental breakdown where the utterances of the patient have shown an outflanking of infantile, i.e., unconscious tendencies. The inner meaning of these precipitating causes has been determined or presumed as follows: We know that with great frequency the content of a psychosis represents a reaction to the precipitating cause. In this way, therefore, we can observe the hidden meaning of the precipitating factor—the patient, in a sense, explains it himself. Again, there are certain symbols which repeat themselves so often that, where patients are not accessible for a complete psychoanalysis, we can take their meaning for granted, for the sake of the study. When these occur as precipitating causes, we assume that the latent content of the symbol is responsible for the upset, and proceed to verify that assumption by examination of the utterances in the psychosis. (p. 298)

MacCurdy’s research depended on close association with his mentor Hoch, and in general, MacCurdy’s psychodynamic speculations were based on the excellent data provided by experienced clinicians, especially Hoch and Meyer.

Both of these pre-eminent psychiatrists had the same point of view in clinical research: that it must begin with painstaking record of what patients say and do, regardless of the part of these observations for any particular theory, and, further, that the descriptions of mental symptoms must be put into terms of common speech, so far as is possible, because technical labels tend to obscure individual differences of reactions. . . Every worker in the Institute was trained to follow these rules. (p. ix)

MacCurdy left the Institute for military service at the time Hoch retired, but he did not give up the research he and Hoch had conducted during their years of association at P.I. Upon being discharged from military service in the spring of 1919, he went immediately to visit Hoch, then in retirement in California, to continue their cooperative efforts. This cooperation lasted until September, 1919 when Hoch died, but MacCurdy continued their research, which ultimately culminated in his book, The Psychology of Emotion, a classic of Psychodynamics. This work was published in 1925, long after MacCurdy’s and Hoch’s formal association with P.I. ended, and yet much of the research and the phenomenological base from which it sprang, are an integral part of the history of P.I.

MacCurdy’s association with P.I. and his dependence on the clinical work of Hoch, Meyer, and others parallels Karl Jaspers’ work at the Heidelberg Clinic at the time Franz Nissl, with others, created a “community of living research” in which “phenomenology and the psychology of meaningful connections came into being.” (p. xii). Both MacCurdy and Jaspers, each in his own setting, were brilliant psychodynamic theoreticians who, either by choice or by force of circumstance, depended on the excellent phenomenological material provided by others, and did not spend many years developing their own clinical experience.

ADOLF MEYER’S PHENOMENOLOGICAL METHOD AND THE STANDARIZATION OF PSYCHIATRIC ASSESSMENT

The Psychiatric Institute has made major contributions—through the early work of men like Meyer, Hoch, Kirby, Cheney, and Lewis—to the development of today’s standardized quantitative methods of evaluation. Although the efforts of these early workers in providing detailed guides for evaluation and diagnosis were primarily clinical in their orientation, they must be recognized for providing the indispensable basis of phenomenological material and categorization necessary for later formal attempts at standardization.

Even during his earliest experiences in mental hospitals before the turn of the century, Adolf Meyer was dissatisfied with the haphazard way in which notes were taken and diagnoses made, and particularly with the custom of relying on unobservable internal physical phenomena presumed to be the etiological factors instead of paying strict attention to the observable facts of each case. By the time he was appointed director of P.I., he had begun to work on P.I., he had begun to work on a method to achieve greater thoroughness and objectivity in the reporting of clinical observations. In 1904 he indicated that a systematization of the phenomenological bases for psychiatric evaluation was necessary before formal standardization of techniques could be achieved.

The feeling that the old method of case books was inadequate had already led to a reform in the shape of a system of blanks a few years ago. I think you all agree with this statement, that the blanks alone are far from bringing clinical salvation. They foster the appearance of completeness, but when you wish to use the results for any specific purpose, you are bound to find many important individual traits of the case rubbed off, and the rest forced into a rigid vocabulary, as rigid as the blank itself. . . . The failure of the older plans led me to recommend the later plan of having the records written out, provided with summarizing headings and underscoring, and to urge that the results of the work shall be submitted to staff meetings for mutual criticism and correction. (p. 122)

It should be pointed out that Pinel was the first in modern times to take careful notes on patient behavior—often written on whatever paper was available at the moment, such as the backs of envelopes—but it wasn’t until Meyer came on the scene that systematic case notes were introduced.

Kirby reports that at about this time Meyer “prepared a set of clinical guides or outlines” (p. 5) which were distributed to state hospital physicians in type-written form. These guides, later printed privately in 1908, represent the first

§The same tendency was evident in experimental psychology when hypothetical physiological processes were invoked as an explanation for behavior, leading Skinner to refer to CNS (Central Nervous System) as the “Conceptual Nervous System.”
2. "Mood," especially degree of affect, manic or depressive behavior, willingness to overcome despondency, anxiety, reaction to irritants.

3. "Traits related to the sexual instinct," such as ability to hold friends, relationships with family members, and attitudes and behavior towards the opposite sex, as well as an assessment of overt sexual behavior and preoccupation with sex-related ideas.


A final section inquires into pathological traits such as "tendencies to hallucinate without definite psychosis ... nocturnal enuresis, tics, etc."

The guides by Meyer and Hoch were revised and combined into a new edition by George Kirby in 1921. During his tenure as director, Kirby included the contents of the anamnesis which had been developed by Meyer specifying details of family and personal history, including intellectual, social, and sexual development. Otherwise, he followed closely the content of Meyer's and Hoch's guides. This work was in turn revised by Clarence O. Cheney in 1934 and 1938 while he was director of the Institute, and then again by Nolan D.C. Lewis in 1943 during his tenure as director.

The successive steps of reediting did not obliterate the foundations provided by Meyer and Hoch, and in fact the continuity through successive editions has demonstrated the soundness of the original guides. Changes tended to add to, rather than alter the original material. The continuity of Meyer's and Hoch's influence through successive editions of the Guides was paralleled by the establishment as a tradition of their standards of phenomenological excellence in record-taking at P.I. An ongoing study by Kuriansky et al. using P.I. patient records, has shown this. In her study Kuriansky sampled patient records from the 1930s and 1940s and submitted them—w ith indications of the original diagnosis deleted—to a group of psychiatrists for re-diagnosis. As part of the study Kuriansky asked the psychiatrists to evaluate their confidence in the quality of the records as a basis for re-diagnosis, with most of them expressing satisfaction with the adequacy and completeness of the records. Kuriansky reports that earlier records, taken when Meyer, Hoch, and Kirby were exerting their influence directly, were fairly constant in their format and exemplary in the completeness of their content.

The Lewis edition of the Guides in 1943 took an important step forward in the trend toward standardization. Joseph Zubin, then Associate Research Psychologist in the Psychology Department, collaborated with Lewis and provided psychometric methods for the evaluation of intelligence and personality, many of which were developed by researchers who had been formerly associated with the Institute. Grace Kent's "Emergency Battery of One-Minute Tests" was adopted for a quick evaluation of intelligence, as well as her "Opposes Test," for testing the ability to maintain an associative trend. F. Lyman Wells' "Absurdity Test" was incorporated for evaluating syllogistic thinking. Items from David Wechsler's Wechsler-Bellevue Test were adopted for detecting impairment of judgment in psychopathic personalities (see the next section for a discussion of their researches). Thus early research opportunities afforded by the Psychiatric Institute and the State Hospitals System paid dividends in later cross-fertilization when these individuals moved elsewhere.

The cooperation of psychology with psychiatry in extending the mental status schedule provided one of the inputs to the psychological testing on the Columbia-Greystone Tompectomy Project (1947-1956). Although the Mental Status Schedule was oriented primarily toward diagnosis, whereas the psych-

The examination consists of a series of tests of efficiency of the various organs and functions, and the coordination of the results for the determination of the types and nature of disorders. . . . The direct findings must be given in exact terms of test and reaction, and in mental examination, to quite an extent, with the verbatim account of question (or prompting) and answer. A stenographic record is frequently essential. (p. 139)

The guide is divided into a physical examination aimed at uncovering the somatic components of the patient's disorder, and an examination of the mental condition of the patient. The physical examination consists of psychophysical tests for the functioning of the sensory organs, and motor tests for coordination and presence of tremors, paralysis, and other somatic disfunctions, in addition to tests and observation of the various organ groups.

The mental examination is composed of: 1) general observations on the patient's adaptation to his environment; 2) samples of his "spontaneous drift of mental activity and utterance;" 3) observation of "special moods and emotional traits," especially affective and delusional or hallucinatory states, topics of rumination, and substitutions; 4) a set of mental status tests; 5) an evaluation of the patient's insight into his condition, and 6) observation of "the evolution of symptoms" over a period of days or weeks.

Of particular interest are the psychological tests, constituting a special inquiry into the condition of the sensum ... as shown in the grasp on the past and present, and including tests for orientation as to place, time, and persons; memory of recent and remote events; retention of fund of information; counting and calculating; reading; enunciation; writing. (p. 142)

Diagnosis and prognosis are based on an analysis of the physical and mental symptom picture in terms of a set of reaction types: 1) "reactions of organic disorders;" 2) "delirious states with dreamlike imaginative experiences, hallucinations ... ;" 3) "affective oscillations;" 4) "paranoic developments;" 5) "substitutive disorders of the types of hysteria ... and psychasthenia;" and 6) "types of defect and deterioration."

During August Hoch's tenure as director of the Institute he edited materials for a guide specifically for the evaluation of personality to supplement Meyer's general guide to psychopathy, and together with George Amsden of the Bloomingdale Hospital he published "A Guide to the Descriptive Study of the Personality" in 1913. The guide is set up as a questionnaire, although Hoch preferred it to be considered "a guide to be modified in individual cases," especially in light of the difficulties met in sizing up affective reactions. A preliminary set of questions aims at an estimate of intelligence and judgment, at the general output of energy in the individual, and at his estimate of himself, i.e., such traits as "self-reliance, self-depreciation, conceit, or self-pity." The body of the guide contains questions relating to the following:

1. "Adaptability toward the environment" in terms of ease of social relationships; aggressiveness; openness to others; exaggeration of traits such as order, and demand for truthfulness; assertiveness; and hold on reality.
logists on the Topecotn project were primarily interested in determining brain function as affected by psychosurgery, the content of the mental status schedule and its trend toward standardization were nevertheless important prerequisites for the later psychological study of brain function.

Zubin also became interested in the possibility of systematizing the results of the Mental Status Examination by providing a schedule for recording the findings. Unfortunately, this was 1944, during World War II, and he had to leave for military service before he could develop the schedule further. Although it was used for a while, the lack of manpower soon resulted in its being shelved.

It was revived in 1960, after the Biometrics Research Unit was established, in the form of a Ward Behavior Rating Scale prepared by Eugene Burdock, Anne Hardesty, G. Hakerem, and Joseph Zubin. Subsequently, urged on by Zubin and with the help of a grant from the NIH to the Unit, a systematic approach was started by Burdock and Hardesty in the development of a structured interview for assessing the psychopathology of mental patients. This differed from the rating scales already available in so far as it involved direct systematic interviewing with regard to specific assessable behaviors that could be noted as present or absent, rather than on overall judgments based on prior knowledge of the patient. Subsequently, Robert L. Spitzer joined this team as a research fellow and eventually Jean Endicott joined Spitzer in the newly formed Evaluation Section of the Biometrics Research Unit, which devoted itself to the preparation of a series of systematic objective interviews and rating scales. At a later date, Gurland and Sharpe formed a section on Diagnosis and Psychopathology and further adapted the available instruments and developed new ones for international comparisons in the bilateral study of diagnosis in the US and UK.

Thus modern research in the quantitative and objective evaluation of the mental status of patients has roots going back to the efforts of Meyer and Hoch to standardize diagnostic procedure just after the turn of the century, efforts that were aimed at eradicating the nineteenth century legacy of diagnostic procedures based on subjective impressions and recorded in terms of fancied neurological dysfunction and degeneration. We have not yet made much progress in uncovering etiology, but we have at least systematized the descriptive approach to psychopathology by providing interview tools that are reliable and valid.

**QUANTITATIVE APPROACHES TO THE STUDY OF WORD ASSOCIATION AND MEMORY**

The three decades from 1902, when Meyer became director, until 1930, when the institute was restructured to include a broad-spectrum research program, were a period of primary preoccupation with clinical work and teaching at P.I. The lack of a permanent research staff in the social and behavioral sciences, due largely to the lack of funding, prevented the development of a substantial program of research. In spite of this, P.I. continued to be involved in research advances through the influence of staff members on research conducted elsewhere in the state hospital system, and by providing a suitable environment for outstanding scientists to conduct research. A discussion of the important research projects carried out in connection with P.I. during this period will exemplify how P.I. continued to exert an influence on research interests.

Meyer's work in psychodynamics brought his attention to the early work done by Jung and others with the word association technique. He had experimented with the association test as a clinical tool. He succeeded in 1910 in getting the funds with which to start a "psychological department" at P.I. and to bring F. Lyman Wells, an experimental psychologist, from the McLean Hospital. Wells concurred with Hoch's interest in using experimental psychological methodology as an aid to clinical investigation.

The proper starting point for experimental investigations in psychopathology is the psychotic symptom itself. Problems must be chosen, first, with reference to actual clinical groups and the pictures that form them; secondarily, to the convenience of the experimental method, and as little as possible with reference to preconceived psychological systems. The nature of such problems can be known in one way only—by actual knowledge of the cases that present them.

There were, in addition, much deeper intellectual ties between the two men. Wells was one of the early proponents of the psychodynamic approach to psychopathology. He participated in the Clark University centennial celebration in 1909 in which Freud, Jung, and Meyer presented their views; later he wrote critical and expository reviews of the psychoanalytic literature for the *Psychological Bulletin*, as well as original articles, in which a major current was his desire to see psychodynamic formulations submitted to more rigorous examination and validation. David Shukov comments that Wells' "considerable role in introducing and reconciling psychoanalysis to academic psychology has largely remained..."
unappreciated and unrecognized." (p. 48). Wells thus came to P.I. with a background and interests promising meaningful collaboration with Hoch and other staff members interested in psychodynamics.

In a paper read before the Ward's Island Psychiatric Society by Wells surveyed the field of psychological research to give a picture of what he thought his department should be engaged in. Among the experimental approaches he mentioned is the association test, used for "the discovery of certain associative trends which may be indicative of a given disease condition" and as an aid in psychoanalysis. Measurement of the psychogalvanic reflex would allow a study of "involuntary responses to given suggestions" in "cases who come under observation totally inaccessible, mute, irresponsible, furnishing absolutely no clue as to their mental content." The study of writing speed and pressure, the speed of tapping, and ability of the "eye to follow a rhythmically moving object, as on a pendulum" may help distinguish "the retardation of the manic-depressive condition and the blocking... of dementia praecox."

Wells felt the study of memory to be of paramount importance in psycho-pathology. He differentiated study of the sense modalities of the stimuli; the study of memory for "the occurrence or non-occurrence of a series of single events;" logical memory, associative memory, and recognition memory; and the study of retentiveness, i.e., the time elapsing between stimulus and reproduction. Experiments along these lines would, he felt, be valuable in the study and diagnosis of senility, "manic cases," "dementia praecox," the Korsakoff syndrome, and general paralysis. In particular, he anticipated the potential of memory testing for differentiating between arteriosclerosis on the one hand and general paralysis and senile dementia on the other.

Wells' own work during his short association with the Psychiatric Institute continued his evaluations of the association reaction test begun at the McLean Hospital. He rejected the phylological approach to classifying responses, as exemplified by the work of Jung and Riklin, an approach which grouped "the reactions in a series of logical categories, based largely upon grammatical conception," because it was "very unwieldy, very subjective, and its interpretation more than ambiguous." In the analysis of association reactions he noticed two kinds of phenomena that, to him, provided an objective classification: 1) "a tendency to react with either supra-ordinate (the non-specific of Kent and Rosanoff) or else with contrast association" and 2) "the extent to which the reactions are influenced by the individual's special experience or judgment." A further step toward objectivity was provided, in his opinion, by Kent and Rosanoff's technique of comparing the frequency of specific response-words to a standardized control population, rather than attempting to classify the responses of the individual.

In his work at the Psychiatric Institute Wells was able to ascertain "the existence of definite types of association reactions in different individuals" which he hoped would lead to a delineation of different personality types. He was also concerned with the effect of practice on association reactions. In working with students of R.S. Woodworth of Columbia University he applied association tests, a tapping speed test, and sensory and memory tests to the study of "manic-depression insanity."

The fates of the two research psychologists in the State Hospitals system during this period seems to indicate that the climate was not yet ready for a permanent research facility. Grace Kent resigned from the staff of the Kings Park State Hospital in November, 1910, just after the publishing of her classic study on word association, to join the staff of the government hospital in Washington, D.C. Wells, appointed to P.I. the same month that Kent left Kings Park, resigned the following July because of "ill health," and returned to the McLean Hospital.

Several years after Wells' departure the Institute again briefly became the focus of psychological research when David Wechsler, the now famous author of the Wechsler Intelligence Test, then a graduate student under Professor Woodworth at Columbia University, came to conduct his Master's essay research under the clinical supervision of August Hoch, who had previously brought F. Lynn Wells to the Institute. Wechsler's study was an investigation of memory in five alcoholic Korsakoff cases and one general paretic. This was one of the first quantitative psychological studies at P.I. and the first investigation of the Korsakoff syndrome in the English literature. He tested four hypotheses about the amnestic syndrome, so prominent in Korsakoff cases: (1) defective perception, (2) failure to link new associations with old ones, (3) rapid fading of newly formed associations and (4) inability of reproduction. Wechsler concluded that the third hypothesis was most tenable as an explanation of the memory deficit—"ability to form durable new associations. This research concerned in the publication in 1917, of "A Study of Retention in Korsakoff Psychosis," reprinted in the collected papers of P.I. Unfortunately, the initial report of this study was buried in the state hospital reports, so that when interest in memory studies following ECT arose, this earlier study was not available as a guide for investigation.

In the early 1920s Gardner Murphy made use of the patients and the nursing staff at Manhattan State Hospital for a word association study of schizophrenics, manic depressives, and normals in which he worked closely with F. L. Wells, who had returned to the Boston Psychopathic Hospital. He used both the traditional classification of responses according to logical type and the methodology of Kent and Rosanoff, and concluded in agreement with Kent and Rosanoff that there is a gradual transition rather than an abrupt change from the normal state to pathological states, and that "types of word association, as such, are but little related to the fundamental attitudes and adaptations to life underlying the mental disorders which are here compared." (p. 571)

THE KOPLEOFF EXPERIMENTS

Popular attention was given in the early 1920s to the focal injection hypothesis of Henry Cotton, which held that the sources of mental disorder to be found in the toxins produced by such foci of infection as infected tonsils, adenoids, colons, dental caries, and so on. Surgical removal of these sources of infection was proposed as a means of eliminating mental disorders. Nicholas Kopleoff, the Associate Bacteriologist at P.I., and Clarence O. Cheney and George Kirby of the clinical service tested this hypothesis in a series of studies which proved to be an important forerunner of subsequent work in the evaluation of therapy. They removed the foci of infection in some 60 patients and compared the improvement rate of this group with that of a suitable control group after a year's follow-up. The improvement rates did not differ (for the "dementia praecox" cases, p = .50; for the manic depressive cases, p = .82; and for the total sample, p = .661). The only difference noted was in the higher mortality rate of the operated group.

The importance of this experiment goes far beyond its refutation of the focal infection theory. As one of the earliest controlled experiments in psychiatry, it...
served and has continued to serve as a paradigm for research into the causes of mental disorder, as well as for the evaluation of therapy. Just one example of the influence of this experiment is to be found in a proposal for the evaluation of psychoanalysis made by C.O. Cheney and Carney Landis. They planned to select a group of patients for psychoanalysis and to match this group with controls who would receive identical treatment, except for the psychoanalytic therapy. Cheney and Landis give credit to the earlier Koploff experiments for this controlled approach.

Several years ago the Psychiatric Institute conducted an investigation of the scientific therapeutic merits of the elimination of foci of infection in the new building and recovery from mental disease. These studies and controls might well serve as patterns for a similar investigation of the therapeutic value of psychoanalysis. [p. 1162]

CARNEY LANDIS’ DEPARTMENT OF PSYCHOLOGY IN THE NEW PSYCHIATRIC INSTITUTE

The dream of an autonomous institute uniting research and teaching functions in the study of mental disorders has long been nurtured by scientists within the Psychiatric Institute and without. In 1920 the first legislative overtures were made. In 1923 the passage of a $50 million bond issue for state institutions made funds available for a new building. In the years that followed, a happy cooperative effort conceived by Dr. Thomas Salmon between the State Hospital Commission and the administrative board of the Columbia-Presbyterian Medical Center was set up, resulting in a restructured and expanded Psychiatric Institute which was completed at its present site on Washington Heights in 1929. The plans of the new Institute included a department of clinical and research psychology, specifying laboratories on the 13th floor.

It is not clear who may have been the moving force behind the inclusion of a psychology department in the plans for the new institute. Certainly the establishment of psychological research departments at other institutions, such as St. Elizabeth’s Hospital in Washington, D.C., Worcester State Hospital, McLean Hospital, and the Institute for Juvenile Research in Chicago, was indicative of a developing zeitgeist in which the techniques of experimental psychology were increasingly important for psychiatric research. Kirby, the director of the Institute, who was asked by the State Hospital Commission to submit plans for the organization of the new institute in 1925. His annual report to the commission in 1926 includes plans for laboratory space for experimental psychology. Later reports mention clinical psychology as well. Given a mandate to expand the facilities and the research staff of the Institute, Kirby no doubt included psychology as a matter of course.

Once P.I. moved from Ward’s Island to its newly built home at the Columbia-Presbyterian Medical Center the sporadic work in research psychology took on a more continuous character. Soon after the building opened, Carney Landis—who had noticed the availability of a research position at P.I. in an advertisement and, out of curiosity, had taken the civil service examination for it, which he passed, to his great surprise—was appointed Associate Research Scientist heading up the newly created Department of Research Psychology. Many of his colleagues in academic psychology, who were surprised by this move, tried to dissuade him by warning him of the political forces limiting the work in institutions and cautioning him that he was giving up what promised to be a brilliant career in physiological psychology. He took the job nonetheless. The premonitions of his colleagues proved to be false when he subsequently received academic recognition through a professorship in the Department of Psychology at Columbia University. In that position he was in charge of teaching in Abnormal Psychology, and was able to recruit many graduate students for his laboratory at P.I. and for dissertations in the field of psychopathology.

When Landis came to P.I. he had already demonstrated his research ability with such problems as facial expression in emotion and in examining delinquent behavior and in a keen analysis of the psychogalvanic reflex phenomenon. He gathered about him a coterie of enthusiastic young researchers including William A. Hunt, Joseph McVicker Hunt, Walter Shipley, Theodore Forbes, and Joseph Zabun and others who began to tackle the various aspects of psychopathology in a systematic manner.

An interesting struggle developed between the clinical service-minded staff, who were largely drawn from state hospital circles and the newly appointed associate research staff, who were primarily raised in academic circles. The clinicians, being in command, expected that the researchers would turn their attention to burning clinical issues such as schizophrenia, while the academicians, accustomed to dictation, wanted to choose their own research areas without reference to local clinical needs. Furthermore, the clinical staff, influenced by such men as August Hoch, John MacCurdy, and MacFie Campbell, were under the spell of psychoanalysis, whereas Carney Landis as well as the other academically trained scientists were not infatuated with psychoanalysis to the same degree as their clinical peers. The struggle often erupted into open hostility. Landis reflected on this in an interview held many years later: 1

The scientific staff was unable to come to terms with what they might do for and with the clinical staff. Only Drs. Ferraro (neuropathology) and Koploff (bacteriology) had come over from Ward’s Island. The rest of us who had worked with the clinicians. The fuss came with chemistry, internal medicine, and my department, experimental psychology. The clinicians wanted us to see patients and gave the benefit of our knowledge. They wanted us to work for them at their direction and at their orders. We fought it out with them and there were bloody heads all around. (p. 51-2)

Since the clinical staff had the upper hand, the scientists soon began to compromise lest their financial support for research be curtailed. Landis apparently was sufficiently flexible to accommodate himself to the clinical demands. He introduced a clinical psychologist—Dr. Zygmunt Piotrowsky—into the department in 1930 to take care of clinical testing and to investigate schizophrenia and subsequently epilepsy and other clinical conditions as the need arose. In this work he was greatly helped by the close cooperation of Sigfried Katz, a fine-trained clinician who had had some psychological training. This was especially fortunate because many of the problems undertaken in psychological psychology would have been impossible to conduct without medical supervision. Landis often discussed the need for a medical degree in order to permit a free undertaking of studies involving drugs or other intradental experiments. He realized, however, that the holder of a medical degree would run the risk of being railroaded into practice rather than research because of the great need for clinical service. He regarded the absence of a medical degree as a defense against being seduced.

One of the first problems that attracted his attention was that of the striking muscular behavior of the catatonics. He examined the effect of oxygen and carbon dioxide on catatonics, the stereotypy of their autonomic responses; he investigated whether their catatonic tonicity disappeared during sleep by photographic
their posture while awake and while asleep, and was surprised to note that though manics were more active than catatonic in their motility, the difference, when measured, was much smaller than had been supposed. This led to a series of studies of muscle tension in normals and patients which yielded no differences. This research in catatonia was a far cry from what the clinicians had expected—since it contributed little to methods for treatment—but it indicated how psychology can serve as the basic science for psychopathology even as physiology serves internal medicine.

Landis did not limit himself to the problems raised by clinicians and, intrigued by Bill Hunt's attempt at replicating Maran's studies of induced emotion by injection of adrenaline, he continued together with Hunt to find that "the injection of sufficient amounts of adrenaline will reproduce roughly the organic picture usually characterized as emotion." (p. 483.) The individual reactions varied widely, however, with some subjects reporting only organic effects, some reporting emotion-like states, and a few experiencing emotions that seemed satisfying and genuine.

The introduction of a children's ward into P.I. engendered research in child development, especially in the area of play techniques and in investigations of emotional behavior. No differences were found between mentally defective children at Letchworth Village and children at P.I. in emotional behavior through the use of Jersild's interviewing techniques.

Because of the use of hyperthermia in treatment of general paresis, psychological and neuromuscular tests were conducted with general paretics and measures of improvement on psychological tests were used in connection with the evaluation of outcome of treatment.

Soon after Hans Strauss described the Startle Pattern in Germany he invited Strauss to come to P.I. to collaborate on further studies of this phenomenon by the use of ultrarapid moving-picture cameras. This classic work appeared in 1939 as the first published volume reporting the results of the laboratory. In general, they found a consistent response pattern to a pistol shot in normals and patients, including eye-blink, distortion of facial features, forward movement of the head, and a variety of body movements.

Landis, Hunt, and Strauss wanted to determine to what extent this response was involuntary, because of a basic interest in the functioning of the central nervous system. In order to determine the effect of habituation on the startle pattern, they visited a firing range of the New York Police Department with their high-speed motion-picture equipment and tested a group of beginners and experienced marksmen. Even those with frequent gun-firing experience showed eyelid, head movement, and facial distortion responses, although some denied it until shown the films. This experiment established the startle pattern as an involuntary response.

In the work with mental patients, no qualitative differences from normals in the startle pattern were found, although the quantitative degree of reaction was somewhat greater, especially in the feebleminded. This difference was attributed to the possibility that the normals were more aware of what was going on, thereby reducing the element of surprise. In epileptics, however, they found a drastic reduction in response level. Out of 109 patients tested at the Craig Colony, 30 showed no reaction at all, not even eyeblink. A more careful analysis of higher-speed film revealed that some of these epileptics were making an almost imperceptible partial blink, but this only served to confirm the qualitative difference between the epileptics and other clinical categories tested, along with the normals.

The findings with epileptics were followed up by testing schizophrenics undergoing experimental convulsive treatment with metrazol, in order to study what happened during seizure. The startle pattern was completely eliminated during a seizure, and returned gradually five to ten minutes after it was over. Other experiments were conducted with deaf subjects, normals given adrenalin injections, and hypnotized subjects. The deaf and those under hypnosis showed a sharp reduction in response.

These findings were used, especially by Strauss, to construct hypotheses about possible neurological processing. No great advance was made in the formulation of a neurological model, but on the other hand, these experiments represented a methodological breakthrough in drawing attention for the first time to the relevance of autonomic responses in the microscopic interval of milliseconds after stimulation.

Another area that occupied the attention of the Department was the general area of physiological psychology, including electrical phenomena of the skin, thyroid gland action, and electrical processes of the cochlea and auditory nerve, in which Landis and Forbes made basic progress; but there was no payoff for psychopathology. Even the auditory threshold failed to differentiate schizophrenics from normals.

Another area that began to flourish in the 30s was that of personality inventories. Together with Page, Benton, and Zubin, considerable effort was expended on developing inventories that would help in screening the mentally ill from the well by self-reporting questionnaires. Although this work did not pay off with psychotics, it did form the basis for screening techniques subsequently used in World War II by Walter Shipley in the Navy and by J. Zubin in the Merchant Marine Service. Zubin, with the help of the War Shipping Administration, developed a screening inventory for merchant seamen which proved very effective in eliminating the unfit sailors from the hazardous duties of the merchant marine during the war. Shipley adopted this technique in the development of his Navy Personal Inventory II, which was widely used during World War II. Both the late Walter Hunter and Clarence Graham regarded it as an important contribution from the OSRD committee. Some basic developments in methods for determining similarities in patterns of response arose from this work and culminated in a method for measuring likemindedness.

Another area in which the department pioneered was that of psychosexual development. This work, beginning in 1934 with a study of 700 psychotic women by Frances Strakosch Alexander, was followed in the early forties by studies of neurotic women and physically handicapped women with Agnes Landis and Majorie Bolles, respectively, as coinvestigators. Interviewing techniques were developed by them, before Kinsey, for obtaining relevant psychosexual information. It was found that the mentally ill did not differ strikingly from the well in early psychosexual development. Despite this, adult patients did exhibit greater deviation from normals in sexual behavior and adjustment.

These studies of psychosexual development were anticipated by the work of C. E. Gibbs, who studied the physical and behavioral pathology relating to sex and development in schizophrenic and manic-depressive patients at P.I. in the early 1920s. His research, in its concentration on the distribution of pubic and mammary hair, seemed to have been based in part on the by then rapidly failing degeneracy hypothesis, and thus did not attract attention. He did use a control group in the investigation, and employed quantitative techniques to analyze the data.

Hardy had a new treatment been introduced when the Department of Research Psychology would begin to evaluate it. First came the treatments for
general paresis in 1934 when electropyrexia, electropyrexia followed by tryparasme combined with mercury, and tryparasme combined with fever treatment were compared, and the second type of treatment found to be superior on the basis of psychological test improvement; following this came evaluations of child therapy, psychotherapy, psychoanalysis, experimental neuroses in children suffering from diffuse difficulties, insulin therapy, metrazol therapy, ECT, psychosurgery, antihistamine, and, finally drug therapy. All treatments were subjected to the crucible of objective evaluation by this department. One of the most noteworthy studies in the evaluation area was the pioneer study on the evaluation of psychotherapy conducted by Landis, in which he reviewed the literature on outcome of this treatment against the outcome of psychotherapy at P.I.58 His findings, which were subsequently confirmed by Eysenck, by Zubin, and others, led to the conclusion that when control studies are considered, the efficacy of psychotherapy remains undemonstrated.

When psychoanalysis became popular enough to require attention, Dr. Landis turned attention to this treatment. In order to be able to assess it properly he underwent a didactic psychoanalysis himself, and in 1940 reported his experience in the series of reports in the Journal of Abnormal Psychology59 in which several other eminent psychologists participated. His critical assessment was one of the first to subject the psychoanalytic process to objective scrutiny.

Then followed a series of studies in the epidemiology of the mental disorders which was reported in a volume with James D. Page entitled "Modern Society and Mental Disease."60 In this book a survey was made of the ecological factors that influenced the distribution of the various mental disorders throughout populations and became a classic in this field.

Among the psychological techniques that the department developed for diagnosis, prognosis, and evaluation of treatment in addition to the previously described personality inventories were sorting tests for investigating thought disorder, handwriting analysis, and the Rorschach technique and other projective techniques. Joseph McVicker Hunt spent the year 1934 as a postdoctoral student investigating thought processes in psychotic patients. When the late Kurt Goldstein arrived at P.I., a whole series of sorting tests were devised or adapted to investigate sorting behavior in schizophrenics as contrasted with general paretics and brain-injured cases. One outcome was a series of scales developed by Zubin for classifying sorting behavior which proved to be prognostic of outcome in insulin therapy. Zygmunt Piotrowski introduced the Rorschach technique and developed it over the course of the years. He found it to be prognostic of outcome in insulin treatment and developed organic indicators as well as a whole series of new scoring methods, especially for movement responses. A series of scales of evaluating handwriting was developed by Thea Stein Lewinson and Zubin. The effect of ECT on memory was investigated by Zubin and colleagues and resulted in the finding that whereas ECT temporarily wets out recall memory, recognition memory is relatively unscathed, although a new phenomenon does appear—that of loss of familiarity. Three weeks after the end of therapy these aberrations were found to disappear.

During the World War II period, research practically came to a standstill, except for the studies in ECT memory loss and personality inventories. Some work was also done on test pilots in the measurement of muscular rigidity at critical points during flight and regarding emotional difficulties interfering with learning. Most of the younger men had joined the armed forces and Landis seized this opportunity to write a textbook of Abnormal Psychology,61 together with Marjorie Bolles, who served as research assistant for many years. The textbooks of abnormal psychology that were available previously consisted primarily of case histories and descriptive approaches. The only numbers to be found were at the top of the page. Landis and Bolles wrote one of the first texts to include substantive experimental data, and it soon became a classic. Zubin became interested in the work on will by Narziss Ach and devoted some months to translating Ach's "Analyse des Willens" into English. This, however, was interrupted by military service.

With the coming of the age of psychosurgery, the department threw itself into the evaluation of this method by joining forces with Fred Mettler and his Columbia-Greystone Associates in the classic topectomy studies.62 The word topectomy was coined by this group to replace the earlier bastard word areaectomy. Fortunately, a group of graduate students freshly returned from the War was available to undertake the task of determining the effect of topectomy on behavior, and the classic series of chapters on psychology in the book "Selective Partial Ablation of the Frontal Cortex"63 consists of their dissertations. This was one of the first collaborative studies in psychopathology and set the pattern for interdisciplinary research in psychiatry by combining the efforts of the following disciplines in an attack on a common problem: neuropsychiatry, statistics, neurosurgery, anatomy, general medicine, psychology, otolaryngology, internal medicine, neuropsychology, neuropathology, and physiology. The results of this investigation and of the two succeeding projects advanced knowledge of brain function dramatically. Although the efficacy of psychosurgery for unselected schizophrenics proved to be doubtful, and although some of the findings regarding the influence of topectomy on behavior have since been challenged, the impact of these studies, in which psychology at P.I. played a crucial role, was quite impressive, especially as techniques of surgery and evaluation alongside physiological as well as psychological techniques became more refined. The failure of psychosurgery as a general therapeutic technique for schizophrenics raised doubts about the efficacy of the other somatic therapies as well, and led to a survey of the literature on all the therapies in 1957.64 The results of this survey, conducted by Virginia Staudt, serving as a postdoctoral student with Zubin, led to the conclusion that as far as immediate outcome of treatment was concerned, each therapy seemed successful, but in five-year followup, none of the therapies seemed to be more efficacious than spontaneous improvement in "untreated" patients.

One of the outcomes of the Columbia Greystone studies was an interest in developing more objective indicators of brain function than was afforded by the clinical tests. The finding that critical flicker fusion (CFF) was influenced by surgical operations on the frontal lobes led Landis to hope that CFF might turn out to be a good indicator of brain function, and he spent almost a decade tracking down the various parameters influencing this function. Unfortunately, this project proved to be rather disappointing in his estimation, and his younger colleagues later demonstrated, using signal detection theory, that the CFF differential between patients and normals was not a difference in sensory sensitivity, but in the criterion adopted by the subjects for deciding whether the light is flickering or fused. While this finding does not eliminate the usefulness of CFF as a clinical testing device, the interpretation of the results is completely altered.

One of the last achievements of Dr. Landis was his book, published posthumously in 1964, on the Varieties of Psychopathologic Experience,65 which attempted to present in casuistic fashion the feelings, attitudes, emotions, and thinking of the mentally disturbed from their own point of view. From a collection of 200 autobiographical books and as many journal articles, Landis drew...
a series of introspective descriptions characteristic of a wide range of psycho-pathological experience. These he analyzed into their underlying dimensions, and developed a systematic view for the subjective experience of mental illness.

The course of development of the Psychology Department at P.I., both in style and in the content of research, was guided for more than three decades by Carney Landis. A student and colleague of his during much of this period, William Hunt, has paid close attention to the course of Landis' career, aptly summarizes his guidance of the department. How best can Carney Landis' contribution to the Psychiatric Institute be summarized? I think by pointing out that his absence of any limiting theoretical bias, his diverse and wide range of research interest, and his continued fascination with and receptivity to the new and novel provided a broad base of intellectual stimulation that enthused his own laboratory, attracted collaborators from without, and encouraged and stimulated work in other departments beside his own. True interdisciplinary research most often arises from the simple overflow of ideas from some investigator whose mind cannot be contained within the narrow boundaries of a single discipline. Landis' could not. Moreover, his tremendous energy assured on the administrative side an expansionist philosophy that guaranteed the continued growth and development (and the feeding) of a sound and adequate research laboratory for psychology. In matters of budget and facilities he was often like an angry mother hen, and equally dangerous, a trait that sometimes did not endear him to colleagues in other departments who had their own chickens to protect. Yet, if Landis believed in another man's work, he was a psychologist or not, he could marshal this same manipulative energy in support of the other fellow. His sense of empire lay in the behavioral sciences in general and not in psychology alone.

But a laboratory ultimately must be justified by its productivity, and this was, and the craftsmanship assured the quality of this productivity and guaranteed its respect. So psychology grew and flourished and the Institute benefitted and grew. The growing interest of a community of interest combined with his insistence on giving his laboratory an air of unorthodox orthodoxy that was both exciting and supportive. It was always fun to be there."

(p. 425-6)

CONCLUSION

Carney Landis died in 1962, leaving behind him a legacy of careful attention to detail in the empirical investigation of psychophysiological phenomena. He also had wide-ranging interests, as evidenced by his pre-Kindergarten work on psychosomatic disorders, the didactic analysis he underwent in order to better approach psychoanalytic methodology, and his late work on the phenomenology of mental disorder. But the main thrust of his career was in the elaboration of empiricist methodology. The pendulum had come full swing from the early days of the “Pathological Institute” under Van Gieson, when the late nineteenth century spirit of mentalism and speculation held sway. Van Gieson did use careful methods in the pathology laboratory to the extent that materials and techniques of the day permitted, but his main interests while at P.I. were centered on developing a detailed speculative scheme linking abnormal behavior in a pre-Kraepelinian categorization to the hypothetical retraction and degeneration of neuron aggregates. The stress here is on "speculative." Van Gieson had neither the laboratory technology nor the clinical observational framework to provide any kind of empirical base for his speculations.

Succeeding generations of scientists at P.I. made advances in these areas. Meyer along with his colleagues and followers established and improved the observational basis for description and diagnosis of mental disorders, in addition to introducing psychodynamic principles into observation and theory. Objective techniques for psychological testing were developed by the Kent-Rosanoff team, by F.L. Wells, and by David Wechsler under the aegis of Hoch and then Kirby at P.I. Kopeloff introduced modern concepts of hypothesis testing to P.I. in his refutation of the “focal infection” theory. In the following decades the empiricism characteristic of Carney Landis' laboratory placed theorizing at the sidelines.

Out of this transition from speculation to empiricism has developed a new synthesis characterized by the conscious construction of scientific models, which are, in a sense, theoretical speculation grounded in empirical investigation. In the forties Franz Kallmann joined the staff at P.I., bringing with him genetic models pertaining to mental disorder, as well as the latest data-gathering techniques for testing these models. Kallmann found a welcome haven in the psychology department until he was able to establish his own department of Medical Genetics. In the fifties and sixties the developing Biometrics Research Unit brought to P.I. a consciously multidisciplinary approach based on etiological models ranging from the genetic, neurophysiological, and internal environment approaches on the one hand, to developmental, learning theoretic, and ecological approaches on the other. The elaboration of these models had consequences for both clinical descriptive practices and for experimental design. In order to evaluate the models rigorously, the need arose for more careful description of psychopathology along the lines that Adolf Meyer had laid down. This need was satisfied by the provision of systematic, structured interview and rating techniques developed by the Biometrics staff. On the other hand, the research programs designed to test hypotheses growing out of these models individually and out of their interactions constitute the main work of the staff. Research interests at P.I. have proliferated, so that now there are a number of new departments taking a variety of approaches to the description and etiology of mental disorders.

If Van Gieson were alive today he might well be regarded as a prophet whose time has come. The modern structure of P.I. and its research interests bear a striking resemblance to the multidisciplinary play Van Gieson developed with Boris Sidis for his Pathological Institute at the turn of the century. The essential differences lie not in the collaboration of sciences, but in the modern spirit of constructing scientific models and empirical techniques for evaluating them.

ACKNOWLEDGMENTS

The authors wish to express their gratitude to Dr. Lawrence Kolb, former director of the New York State Psychiatric Institute, for his initial support and encouragement of their project, and to Mr. James Montgomery and staff of the Institute library for their tireless efforts in making available a wealth of material relevant to the history of the Institute, as well as for their friendly support and encouragement.

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